istic implications as the orthogenesists claim to be from neo-vitalistic stigmata; that Socialists of the type of Hillquit are not anarchists and that a very pretty fallacy underlies the assertion that in the Socialistic state all incentive to invention will vanish; that one can scarcely be at the same time a neo-Kantian and a scientific ethicist. What is further aimed at is to teach the scientific or engineering freshman whom nature has endowed with brains the ability to express his inductions or deductions in readable terms—to, well, let me suggest, write upon Mendelism after the *rhetorical method* of Punnett, and not after that of —. The blank is not hard to fill. If scientists are ever to slay the religion which Huxley likened to Bourbonism, they must be capable of approaching the public with other explanations of abstruse matter than such mathematical exposition as even Professor Bateson admits he "could not follow."

And at this point I verge on my final plea for the use by instructors of rhetoric of some such book as Steeves and Ristine. With all humility and yet all firmness, I contend that the proper teacher of such courses is not the ordinary composition instructor, aided by casual, if expert, colleagues from the other schools, nor, above all, the man with training narrowly limited to science, engineering, or law, but the rhetoric instructor who is wise enough to assign only such topics as he himself has taken the trouble to master. Why not the ardent young scientist? Because the very reason for rhetoricians adopting the new text is that they may train the scientists of the next generation to learn to use the language that seemed adequate to Darwin and Huxley, Smith and Galton, Tyndall and Faraday. I rather suspect that a certain professor of physics was not entirely alone when he so surprisingly confessed in the preface to his well-known book that "he trusted he had made no more errors than he had hoped for." There is, however, a further reason for the objection to turning such courses over to scientists. Scientists love theories and even hypotheses: witness the pleasing manner in which Eimer flayed Nägeli for approximating neo-vitalism—and then note how charmingly mystical is Eimer's own analysis of orthogenetic forces. The basic thing in these thought courses is that there be no adherent to this school or that supervising the course. For, whenever the mere imparting of information or speculation is allowed to take the place of the study of coherent arrangement of material and sharp criticism of independent thought, then the chief value of such courses is thoroughly vitiated. And yet, if rhetoric instructors do not awake, some time or other scientists, engineers and lawyers will somehow face the problem of themselves instilling the principles of unity and coherence into their promising students.

Middle West

SCIENTIFIC BOOKS

Problems of Science. By Federico Enriques. Authorized translation by Katharine Royce, with an introductory note by Josiah Royce, Professor of History of Philosophy at Harvard University. Chicago, The Open Court Publishing Company. 1914. Pp. xvi + 392.

Among mathematicians Enriques, who is professor of projective and descriptive geometry in the Unversity of Bologna, has long been favorably known for his contributions to geometry, especially for his admirable treatise on "Projective Geometry" and for his penetrating essays on "The Foundations of Geometry." In the work before us the distinguished geometrician addresses a far wider circle of students and thinkers: not only mathematicians, but psychologists, logicians, philosophers, astronomers, mechanicians, physicists, chemists, biologists and others. For the discussion, which is as wide-ranging as the philosophic writings of Henri Poincaré or as that of John Theodore Merz in the first two volumes of his "History of European Thought in the Nineteenth Century," deals with fundamental questions drawn from every large department of modern science.

The original text, "Problemi della Scienza," was published in 1906 and has since appeared in German and French translations. Many a

student will feel grateful to the translator and the publisher who have made the work accessible in good form to those whose reading is necessarily confined to the English language.

The work is, in the best sense of the term, a philosophical work. Accordingly, one can not but wonder a little why the author did not choose to call it "Philosophy of Science" instead of "Problems of Science." Perhaps the decisive consideration was similar to that which led Messrs. Whitehead and Russell to entitle their great treatise "Principia Mathematica" instead of "Principles of Mathematics": they feared the warmer title might attract many readers incompetent to understand the work. Doubtless Professor Enriques desired his work to engage the attention of men of science, and he may have reflected that most of these gentlemen are rather repelled than attracted by titles in which the word philosophy occurs. Is our author himself a member of this majority? His evident great care not to be fooled by words or to be lost in nebulous generalities seems to indicate that he is. Confirmatory indicia are to be found in some passages of the work. It is essential "to eliminate all transcendental processes of definition and of reasoning," says Cesaro in the beginning of his lectures on the infinitesimal calculus. Enriques quotes those words of his fellow-countryman and heartily approves them (p. 16) as designed to warn the student "to banish from his mind all metaphysical ideas"! Again, p. 31: "Metaphysics not only puts together symbols without sense, but," and so on. Again, p. 208: "And precisely to ignorance of this subject (modern geometry) are due those strange conclusions over which some philosophers are still toiling." Once more, p. 308: "But even if these objections were not manifest, of what use is it to confute a philosopher? Schopenhauer said nothing could be easier or more useless." Just why the testimony of Schopenhauer is adduced is not quite evident unless it be on the principle that it takes a philosopher to catch a philosopher. One who has attended meetings of philosophic associations and meetings of scientific associations can scarcely have failed to notice this very significant difference: at a meeting of scientific men, when a paper is presented, the author's colleagues assume that the author has probably made a contributon of some value and that it is their privilege and duty to understand it and sooner or later to estimate it; at a meeting of philosophers, when a paper is presented, the author's colleagues usually proceed at once to discuss it with the air of "of course the author's contentions are erroneous and it is our privilege and pleasure to show that they won't bear criticism."

That Professor Enriques should not wish to pose as a philosopher as distinguished from the character of man of science is indeed entirely understandable. Yet his work is a very important contribution to the philosophy, the methodology, the epistemology of science, and, whether or not he would own it, he has shown himself to be a philosophic thinker of immense learning and of great power both critical and constructive. But what kind of philosopher is he? To what school does he belong? Is he a realist or an idealist or a rationalist or a pragmatist or an empiricist or a positivist or some other variety? The answer is that he is at once all and none of these things. He is too big to belong to any of the schools. His thought goes crashing into and through all of them, and, when he has passed along, the scholastic architectures look much as if they had been struck by a discourse of Henri Poincaré. One can not paste a label on Enriques and then inform people of his doctrine by pointing to the label. The only way to ascertain what his doctrine is is to read and ponder what he has said. But who *can* read it? Not many know enough to read it all, but there are many qualified to read it in part, some this part, some that, some another. Even historians (whose province includes the whole activity of man and nature) might try it; so might sociologists, lawyers and men of letters. Should they fail to understand it—well, the consciousness of one's limitations is not always unwholesome, and if it become unbearable, one can take refuge in the soothing reflection that it was Leibnitz who was "the last of the universals."

The author's aim is to contribute to the advancement of epistemology. It is not, however, epistemology in the Hegelian sense. For Enriques, epistemology has for its object "to explain the process by which the most advanced science is built up." It is, he says, "of the first importance that epistemology should be conceived as an actual positive science"; a science in the making, he, of course, means, as is abundantly evident. In a word, epistemology is to be conceived as the science of knowledge, and no one knows better than our author that to make a contribution to the science of knowledge demands knowledge of science. He would probably not deny that, as Thomas De Quincy so well said, every problem of science ultimately roots in metaphysic. But he is convinced that it is not therefore necessary or profitable to be always burrowing like a mole in the black soil where the roots are hid. Bergson the book does not know, probably because the Frenchman's splendid star had not yet risen when the book was written. Doubtless he would agree with Bergson that after the method of science has said all it can of a given object there remains in it an untouched residuum—something of which it is possible and desirable to gain that kind of knowledge that one means when, for example, one says of one's self: I know how to move my arm. Perhaps the Italian would agree with the Frenchman that there is thus indicated a proper province and task for metaphysics, namely, the province and task of winning that residual kind of knowledge through a kind of "intellectual sympathy" with the object, through a kind of fellow feeling with it. But the Italian's epistemology is a different sort. It is "positive" epistemology. It has "a real object to explain." This object is the upbuilding of what we call scientific knowledge and so it has "actual problems to solve." These "ought not to depend upon the inconstant opinions of philosophers" nor "upon the *social* interests that determine these opinions." Epistemology becomes "positive" only in so far as it is established "independently of metaphysics." For Enriques the supreme desideratum in this enterprise is "systematically to banish whatever pertains to the transcendental process of the reason."

What is this dread process? It shows itself in many guises, most commonly, perhaps always in last analysis, as a subtle assumption that an infinite series has in some way a final term, or, if not a final term, at all events an actual limit. In this way all sorts of absolutes, absolute motion, absolute substance, absolute time, absolute morality, and so on, come to figure in our thinking. Such absolutes may have emotional value and so constitute "a problem for the psychologist" but as concepts for scientific use they are worse than worthless. We can not even show that an infinite sequence has a limit by merely showing that it neither diverges nor oscillates.

One of the best sections of the introductory chapter is that in which is dicussed the question of "so-called insoluble problems." It is contended that "in a broad sense there are no insoluble problems." "There are only problems not yet suitably stated." Some one ought to write a work on the history of curiosity. Why have questions arisen in the order in which they have arisen instead of some other order among an infinite variety of thinkable orders? Why have questions seemed to be questions when they have really not been questions? Our author's thesis respecting insoluble problems is well illustrated by him in connection with an admirable account of the famous so-called problems of squaring the circle, perpetual motion and alchemy. This chapter is mainly concerned, however, with the distinction between subjective and objective in scientific knowledge. It is argued that both kinds of elements enter into all scientific knowledge, but as such knowledge advances the subjective component tends to disappear and the objective comes to be more and more. In fact, the two elements "are not two irreducible terms of knowledge, but they are rather two aspects" of it. The question is considered in relation to measurement and to scientific construction. This leads to a critique of positivism in relation to metaphysics, to physics, to biology, to psychology, to history

and to sociology. The entire critique, in which the doctrine of Comte is carefully appraised, hinges on the proposition that, "Strictly speaking, a theory can not be called positive, unless it consists purely of verifiable hypotheses." Those who hope that psychological problems will ultimately receive physiological solutions are not encouraged. The same may be said of those who seek an exclusively economic explanation of the facts of history.

The second chapter (of nearly 50 pages), which deals with "facts and theories," opens with a discussion of dreams and reality. What is reality? What is its criterion? To make a genuine contribution to the literature of that hoary question is something of an achievement. Enriques has made such a contribution. The conclusion is that "the true characteristic of reality is the correspondence of the sensations with the expectation." Reality is thus defined as an invariant, a mathematical term that is gaining currency in various branches of natural science. "There are certain fixed groupings, independent of us, among our actual or supposed volitions on the one hand, and the sensations produced by them on the other. These groupings correspond to what we call the real." The real thus is "an *invariant in the correspondence between volition and sensation.*" The definition involves a hypothetical element: it is presupposed that actual sensations would recur if their conditions were reproduced; but such reproduction is frequently impossible. This conception of reality is examined in relation to the past, to psychology, to society, to biology, to physics, to astronomy and so on. What of hallucinations? The problem is frankly recognized but no pretense of a solution is made. A valuable suggestion, however, is offered. It is that "the patients are unable *to doubt* and so submit their false impressions to a *critical proof directed by the will.*" The object of an hallucination is unreal because the subject's deception is real. How does knowledge pass from common facts to scientific facts? The answer is: by passing from the subjective or individual view to the objective or social view, from the personal to the impersonal view. A common fact is a fact viewed in relation to the beholder; a scientific fact is a fact viewed in relation to surrounding facts. "If I strike a copper plate with a hammer, the plate grows hot," is a common fact. "Bodies are heated by percussion" is a scientific fact. Thus the conception of scientific fact merges into that of law. What is the relation of hypothesis to scientific knowledge or knowledge of reality? "To make an hypothesis signifies: (1) to expect or to foresee given sensations under certain future conditions; (2) to arrange among the groups of actual or controllable sensations, an intermediate grouping which shall serve to associate them in a given order of prevision." This view of the function of hypothesis is elaborated very instructively in connection with such topics as the value of scientific knowledge; knowledge by means of concepts, empiricism and rationalism, the acquisition of knowledge, scientific theories, the theory of gravitation, the electrostatic theory of Poisson, the theory of solutions and the economy and the psychological development of theories.

This many-sided critique of the scientific rôle of hypothesis leads naturally to the question of the offices of induction and deduction in epistemology, and the third chapter (72 pages) is accordingly devoted to problems of logic. To the oft-repeated stupid charge that formal reasoning can not lead to gain of knowledge, our author justly replies that such reasoning serves as an instrument of transformation which, though it does not alter the conceptual data of knowledge, but leaves their truth or falsity to be shown by other means, yet establishes a connection whereby the truth or falsity of certain data implies the truth or falsity of other data. For example, formal logic may show that an hypothesis H implies a consequence C, and it often happens that we can test C directly and thus test H indirectly. The work of induction and deduction is team work. Science can not dispense with either of them. The importance of modern developments in symbolic logic is recognized. An exceedingly valuable discussion of the nature,

function and varieties of definition is given. Every college, and especially every university, ought to give a course of lectures on the subject of definition. There is scarcely any other important scientific subject of universal interest respecting which educated people know so little, but they are not aware of it. How does abstract logic get applied to reality and what are the limits of such application? This very difficult question is examined under many aspects and in many concrete connections: logical representation and the postulate of knowledge, substance (matter and energy), cause, actual value of logical principles, the value of logical principles, the objective reality of logic, the problem of verification, the verification of explicit hypotheses, the experience of a finite number of objects, experience of the continuous, the postulate of continuity and the psychological representation of cause (why and how), the confirmation and verification of implicit hypotheses, the present crisis in political economy, the vicious circle in science and the physiological aspect of logic.

There follows a chapter (59 pages) devoted to geometry. Geometry is viewed, on the one hand, as a part of physics, and, on the other hand, as a purely abstract science. In the latter sense it is a prolongation of logic. Perhaps the most striking thesis in a thoroughly up-to-date discussion, rich in suggestions and insights, is found in that section which deals with the parallel between the historical development and the psycho-genetic development of the postulates of geometry. The thesis is: "*The three groups of ideas that are connected with the concepts that serve as a basis for the theory of the continuum* (*Analysis situs*), *of metrical, and of projective geometry, may be connected, as to their psychological origin, with three groups of sensations: with the general tactile-muscular sensations, with those of special touch, and with those of sight, respectively.*" There be psychologists, and some educators, who think mathematics is so detached from reality as to be an inferior discipline. We should be much interested if these gentlemen would favor us with an expert opinion regarding that thesis of Professor Enriques.

A chapter of 64 pages on mechanics regarded as an extension of geometry is followed by a final chapter of 88 pages on physics in which the leading question concerns the extent in which physics may be regarded as an extension of mechanics. An admirable review and critique of the conceptions and principles of classical mechanics and classical physics in their relation to the new more or less speculative ideas lead to the general conclusion: "*Physics, instead of affording a more precise verification of the classic mechanics, leads rather to a correction of the latter science, taken apriori as rigid.*" The wide range of the author's interest and thought is specially indicated by the closing pages, which are devoted to the mechanical hypothesis and the phenomena of life. The conclusion is that, "in the actual state of our knowledge, *the mechanical hypothesis does not appear to be incompatible with the phenomena of life, but it is unimportant for the study of these phenomena.*" The student will find it instructive to compare the conclusion and the temper of the related discussion with the temper and conclusion in Dr. Crile's "A Mechanistic View of Psychology," published in SCIENCE, August 29, 1913. In this connection one should consider an article by Professor W. B. Smith, entitled, "Are Motions Emotions?" published in the *Tulane Graduates' Magazine* for January, 1914. An even more significant deliverance by the last-named author dealing with the claims and limitations of the mechanical hypothesis is an article bearing the title "Push or Pull?" published in the *Monist*, January, 1913.

In a review of moderate length it is not possible to give an adequate account of Enriques's book. We know of no other work that gives so keen a sense of the unity of all branches of science. A final word as to its manner. The section headings are too numerous, breaking the continuity of the reader's attention; and there are some obscure sentences and paragraphs. These are external faults and are trivial in relation to the inner excellencies of the work.

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